THE AMERICAN

JOURNAL OF PSYCHOLOGY

VOL. I FEBRUARY, 1888 No. 2

ON THE RELATION OF NEUROLOGY TO PSYCHOLOGY.

BY HENRY H. DONALDSON, PH. D.

In the following pages it is proposed to make some statements regarding the relation of neurology to psychology as viewed from the standpoint of the former, and also to give a résumé of some rather recent neurological investigations.

Anatomical results have a reputation for superior credibility, and it is a generally accepted idea that within the limits of gross anatomy this reputation is well grounded; but when we glance at the work in minute anatomy or histology, it seems as though a long time must elapse before this latter would be thus honored. The field is clearing, however, and many existing contradictions are rather apparent than real.

As the result of a long discussion in his last paper, v. Gudden[1] nevertheless reaches the conclusion: "First of all let us have anatomy, or if physiology must come first, then never without anatomical control." In the

study of the form and functions of the nervous system there is reason, then, for accepting the anatomical data—though they are far from perfect—as the norm, and for measuring physiological results by them. It follows from this that the more accurate the standard the surer the progress.

The anatomy of the central nervous system is to-day largely topographical. The units are groups of cells and bundles of fibres, and the question to be answered is, as a rule, how these are united. The methods employed to determine the correlation of function with form are the association of the failure of some function with the failure of some part, or the development of the function with the development of the part. At the outset this investigation is balked both from the clinical and experimental sides by those cases where loss of nervous substance is not apparently associated with any or with a corresponding functional disturbance; but these cases, anomalous as they at first appear, will without doubt become clear under proper anatomical investigation, coupled, as it must be, with a much finer analysis of function than is now in use. It is in this direction that much is to be hoped from the improvement of psycho-physical methods.

Leaving those experiments which have localization as their aim, there arises a further question. Given a group of cells with a definite function, what is the fundamental difference between this group and others with a different function ? This leads to the much less worked, and as yet less fruitful, field in neurology—the characteristics of the cell itself.

The first steps here are the study of the form and chemistry of the cell. We need not stop at this point, however, but at once go on to ask how far we may

reasonably hope to carry this investigation—to inquire what aid psychology is to expect from neurology in ultimate analysis, and whether it will ever come to pass that from the morphological characteristics of the cell its function can be inferred. It must be admitted that thus far the attempts to infer even from the general form of the brain to the grade of intelligence have failed, and yet that appears to be one of the least difficult questions that wait for an answer. It may therefore be asked whether, without the hope of being able to solve any world-riddle, with failures in correlation in full view, is it worth while to pursue neurology as an aid to psychology? An answer based on the following facts may be given in the affirmative. Neurology is the common factor of most of the approaches to psychology. The comparative, the developmental, the anthropological, the morbid, and the experimental methods all have it as a point of departure, and it may fairly be assumed that further anatomical knowledge will give us an insight into the working of the central nervous system far deeper than that which we now possess; that we may learn to associate certain cell changes with the exhibition of certain functions, and that at present we have no reason to fix a limit beyond which the method may not be carried. Accurate neural anatomy must therefore go hand in hand with accurate psychological analysis.

An account of some recent observations in this field, though fragmentary, may serve to illustrate the tendency of investigation in it. The contributions come from various sources. The anatomists, the physiologists, the psychiatrists, and the morphologists are all concerned. The physiological work has thus far been crippled by the lack of anatomical control; the pro-

fessed anatomists and morphologists have but partially specialized in this direction, so that most of the detailed studies have come from the psychiatrists, men who have other engrossing calls on their time, and whose anatomical work must therefore suffer from discontinuity.

The studies on the physiology of the frog's brain by Steiner,[2] of Heidelberg, which appeared in 1885, mark a distinct advance in the physiological methods. They are careful and accurate, and he sharply distinguishes between facts and theories. The chief technical aim of this investigator was the highest experimental accuracy and the reduction of the non-conforming cases to zero. In this he has succeeded admirably. A similar series of observations on fish,[3] some preliminary account of which has already been published, will probably appear soon. Through these results it is evident that the idea of the development of function has gotten a hold on physiological work. A mammal can be kept alive but a short time without its cerebral hemispheres, while Steiner has shown that the fore brain can be removed from a bony fish and yet the latter moves about and feeds in apparently a normal manner. It is to be remembered that in the teleosts the mantle of the fore brain is a non-nervous sac. Is it to be concluded from this that in the bony fish the voluntary control is seated in some part of the brain other than the cerebral hemispheres, or that we have in these fish an animal that is mainly reflex, with a grain of voluntary control? Both ideas are probably correct. Reflex and voluntary centres have differentiated out of more generalized nervous matter, and as this fish has no functionally developed organ corresponding to the usual seat of the voluntary impulses,

it is conceivable that the lower centres of the brain which are alone functional have a more generalized activity, and that they may be the seat of the imperfect voluntary impulses which appear to exist in this animal. So that, looked at from one point of view, the animal is mainly reflex, that is, the voluntary impulses are quite subordinate, while looked at from the other point, such voluntary impulses as it does possess have their seat in sub-cerebral centres. We have, then, in the development of the nervous system in the animal series, not an evolution in the sense of Bonnet and the older naturalists, namely, differences of bulk, proportions remaining the same; but all through there is a most marked alteration in the relative development and physiological value of the different parts. It is an idea as old as Aristotle, that animals possess but one adequate means of defense, or, to put it more generally, have but one apparatus for doing a given thing. The activities of an animal are partly reflex and partly voluntary, but the proportions vary in each case. In passing from a fish to man we are struck by the increase in the voluntary control, and, according to the principle above stated, we should expect—what we find—a most notable decrease in reflex development. If that were not the case we should have in man the nervous system differentiated along two lines, that is, a double apparatus for the same thing, which approaches the impossible. This idea has its application to the recent work on localization. Without the visual centres in the cortex a man cannot see at all; a dog can perhaps see a trifle, a rabbit more, and so on. This fact receives its explanation in that, the more the functions are pushed into the cortex, the more the lower centres become dependent

on it for their very existence, so that removal of the cortex is followed by the degeneration of these lower centres in man and the higher mammals, while, as we descend the animal series, these centres become less and less dependent on the cortex and better able to act alone, as in the frog and fish. Thus the ability to use the lower centres of vision alone appears inverse to the cortical development.

A justifiable complaint is made against the lack of anatomical control in the majority of the physiological experiments. This is due to the practical difficulties in making such a control, which are plain to any one who knows what control means. The matter has been of late slightly improved, both Munk and Goltz having turned over the brains of carefully observed animals to anatomists. But this at best is the sporadic exhibition of a virtue which should be systematic. It is in a high degree naïve for an operator to maintain that a given animal kept alive after operation has suffered a loss of brain substance which is fully represented by what was removed at the operation, and then to make this assumption the basis for further inference. In every case the secondary degenerations must be considered, and the laws of secondary degeneration from the cortex are not yet well enough known to permit in most cases a reliable inference from the injury alone. Even the brains of dogs operated on by Munk, where the operation is precise and elegant in the highest degree, show changes which can only be made out from sections.

To turn to the anatomical side. It is from the method of v. Gudden that the most is to be hoped at present, or it would be, perhaps, better to define it as the "experimental-anatomical method," for, strictly speak-

ing, that of v. Gudden implies the use of new-born or very young animals, and the results thus obtained differ somewhat from those gotten by experiment on adults. The gist of this method is the artificial development of atrophies. But the very means which is here used to unravel the course of the fibres in the centres is itself a legitimate object for experimentation. Far too little is known of the real significance of degeneration to make it a perfectly safe means by which to study other problems.

While speaking of methods we may mention that of Flechsig, or the method of tracing fibre bundles by the development of their medullary sheaths. The assumptions on which this proceeds are that the fibres forming a bundle physiologically distinct assume their medullary sheaths at one time, and that the fibres surrounding them, but physiologically different, do not get their sheaths at the same time. These assumptions are, however, largely open to investigation, and it would increase confidence in the results thus obtained if the details of the process were more clearly known.

As to anatomical results, those of v. Monakow(4) are specially important, from the care and deliberation which characterize the work. In rabbits he has demonstrated the representation of the cortex in the optic thalamus. When a portion of cortex is removed, a part of the thalamic ganglia atrophies, and the relation is a fixed one. So far as tested, similar relations hold for man also. The idea of such a representation had earlier been put forward by Luys, but without any experimental proof. This demonstration is very suggestive, for not only have we the notion of the representation of the cortex in the optic thalamus, but, as cells in the latter atrophy on the removal of the

cortex, they cannot, in mammals as low as rabbits, functionate alone, and must be regarded as intimately associated with the cortex.

General notions of much value have come from Gaskell's[5] attempt to bring the sympathetic system into line, and the introduction of some order and method into that portion of the nervous system has been a very solid contribution. At the same time the spinal ganglia, and those along the cerebral nerves and in the sympathetic, are still riddles, and while they remain without explanation, it may seem a trifle overhasty to rush on to the anatomy of such complicated structures as the brain and cord. No one doubts the value of comparative anatomy for this work, but it might be wise to extend the comparison line into the invertebrates. Dominated by the medical idea that the study of these things rapidly decreases in value as we pass from man downwards, and having a more or less defined feeling that the invertebrates were intended by Providence for the use of the morphologists alone, neurologists have until recently neglected this division; yet it is in the arthropoda that the widest divergence in sense development among nearly related forms is to be found, correlated with differences in the supraoesophageal ganglia. This ganglion, in some ants, has a cortex, and in so far deserves to rank as a brain.

To those acquainted with histological literature the work of Golgi[6] is probably familiar. The essential point of his technical method is the staining of nerve cells with silver nitrate or mercuric chloride, while the surrounding substance remains unstained. The cell and its prolongations thus stand out like a silhouette. The cell structure is unfortunately lost by this treatment, but the prolongations are beautifully exhibited.

The notion of cell prolongations and their significance, previous to Golgi, may be stated as follows: These cells were considered to have one unbranched prolongation, the axis-cylinder process, which continued itself into a nerve fibre. The other, the so-called protoplasmic processes, were branched and were thought to anastomose. Golgi's preparations show first that the axis cylinder, which is wonderfully demonstrated, is always branched, and second, that the protoplasmic processes do not anastomose. Since they generally trend toward bloodvessels, Golgi looks upon them as nutritive arms.

To return to the axis-cylinder processes: These are not all alike. Two types are recognized, and the typical forms are perfectly distinct. The first is that in which the axis cylinder, maintaining its identity, gives off fine lateral branches. The second is that in which the branching is profuse and rapid and in a short space the identity of the axis cylinder is lost in a network. Cells of these types are found separated in the different parts of the brain. On examining the spinal cord it is found that the cells in the anterior cornua belong to the first type, while those in the posterior belong to the second. It was this observation which led Golgi to develop the theory that the cells of the first type were motor and those of the second sensory in function. For the moment accepting this view, there still remains to be explained the meaning of the branches from the axis cylinder and the manner in which the cells of the second type are joined with the periphery. Golgi thinks that it must be through the anastomoses of the branches from the axis cylinder that the several cells are put into communication with one another. All the branches from the axis cylinders

of the first type are thus used, but only a part of those of the second. When an entering fibre loses its medullary sheath it becomes essentially an axis cylinder, and by this method stains as such, so that fibres in this condition can be followed in the neighborhood of the cells. In the posterior cornua of the cord there are fibres which thus enter and break up into a network analogous to that of the axis cylinder of the cells of the second type. It is supposed that this network is in connection with the network of the cells, and that at this point the isolated conduction ceases and the fibre is connected not with one cell but with a group. According to this idea we have to deal with groups of cells instead of single ones, and the explaining power of such a view is considerable. It would have been scarcely worth while to dwell on these observations of Golgi if they were questionable. So far as his facts and plates go they are perfectly reliable, and he understands as clearly as others where the facts leave off and speculation commences. These results have gotten into our literature only somewhat recently, and in order to show that the ideas have not grown up in a night, it may be mentioned that Golgi's first investigation with this method was published in 1874, and from that time up to 1886 he has been more or less occupied in these researches.

This method is selective. Not all the cells are stained; in fact, as a rule, only a small proportion is affected. If now the reaction is a chemical one, it is to be inferred that those cells which stain are in a peculiar chemical condition, and this suggests that changes in the nutritive state of brain might alter the manner in which it is stained.

In this connection at least a reference should be made to the modifications of the above method by

Mondino, a pupil of Golgi. He found that if sufficient time was allowed, whole human brains could be stained throughout by mercuric chloride. It is claimed that this will give a means of following out cell connections equal to any now used, but thus far it has not been put to the test.

It was necessary to wait until something had been said of the experimental method in anatomy and of Golgi's results before referring to the conclusions of Forel,[7] which involve both. Making use of two guinea pigs, he removed the facial nerve in one by pulling it out at the point where it emerges from the base of the brain, and in the other where it emerges from the skull he sectioned it. In the first animal the nucleus atrophied completely; in the second there was some atrophy, but many cells and fibres were simply shrunken, the difference between the two nerves and nuclei in the two animals being very marked. His conclusion from this is that the cell and its fibre considered as one organism can, like any other individual, lose a part of itself without being killed or seriously disabled, but as the relative size of the part removed becomes greater, the injury to the remaining part becomes more marked, until finally, if enough be removed, the remainder will die. According to this view, when the facialis has been separated at the base of the brain, enough has been removed to kill the remaining part, but when it is separated at the point of emergence from the skull, the part removed is sufficient to cause only a severe disturbance in the fibres and cells, and not their death. As is plain, this is rather a suggestion than an explanation; but it is a valuable one. Here is an attempt to give some meaning to the "nutritive centre" theory and the notions of degeneration involved in it.

To return for a moment to the fibres ending in a network, which Golgi finds in the posterior cornua of the cord. Most probably, according to Forel, these fibres arise from peripheral cells, and the network represents the termination in the cord of a fibre which has travelled from a peripheral cell through the posterior roots to the cord. But this is analogous to the cell of the first type, which sends the main stem of its axis-cylinder process directly by a fibre to a muscle, there to end in a network. The picture that we have then, in its simplest form, is that of a central or motor cell sending its axis cylinder toward the periphery and there forming a network, while the peripheral sensory cell sends its axis cylinder toward the centre and there likewise forms a network. Both sensory and motor cells are then of Golgi's first type. But there are still the cells of the second type left over—cells which may be conceived of as modified from the first by the excessive shortening of that part of the axis cylinder which would represent the nerve, thus bringing the terminal network close to the body of the cell. These need to be accounted for. If Forel's idea is correct they are to be designated as central cells, the function being indeterminate. Their relation to the fibres is further borne out by the way they react when the nerve is sectioned. It is known that the disturbance caused in a nucleus by the removal of a sensory nerve differs from that following the removal of a motor nerve. In the former case it is rather a shrinking than a true degeneration of the cells which occurs. This is somewhat explained by supposing the connection with the fibres to be that assumed by Golgi and Forel, or at least by supposing the manner of connection in the two cases to be different. Objections are not wanting

to these views, but at the same time they are supported by the recent observations of His[8] on the human embryo, where the developing sensory cells in the spinal ganglia send their processes in toward the centre.

The above summarizes, in a way confessedly incomplete, certain recent advances in neurology, with a view to indicating what the field is and what some of the results are. If the idea be admitted that pyschic activity is conditioned by anatomical structure, then these results have a significance for psychology.

AUTHORS CITED.

(1) v. GUDDEN.—*Zeitschrift für Psychiatrie*, Bd. 42, 1886.

(2) STEINER.—*Physiologie des Froschhirns*, 1885.

(3) STEINER.—*Sitzungsber. d. Akad. d. Wissen.*, Berlin, 1886.

(4) v. MONAKOW.—*Archiv für Psychiatrie*, Bd. XII.

(5) GASKELL.—*Journal of Physiology*, Vol. VII, No. 1, 1886.

(6) GOLGI.—*Sulla fina anatomia degli organi centrali del sistema nervoso*, 1885.

(7) FOREL.—*Archiv für Psychiatrie*, Bd. XVIII, H I., 1887.

(8) HIS.—*Abhandlungen der Königl.-Sächsischen Gesellschaft der Wissenschaften*, Bd. XIII, 1886.